ments), but in other cases (including the tones of the siren, and the human voice) it depends on a very different cause, namely, the mathematical law that every series of precisely similar vibrations is either simple, or compounded of one set of simple vibrations, giving the fundamental tone, and other sets of simple vibrations giving the harmonics." Now the second cause—Fourier's law—is surely the only one for the existence of harmonics, and in every vibration, whether of a string or any other body, the ear analyses the motion of the air in accordance with the law, that is, hears harmonics. Is it possible that the origin of the misconception here lies in the old notion that when a string vibrates in any regular manner there are secondary waves riding on the primary ones, another set on these secondary, and so on? as—

> "Great fleas have little fleas
> Upon their backs to bite 'em,
> The little fleas have lesser fleas,
> And so ad infinitum."

By the way, why are the notes of the human voice, which are produced by the vibrations of the vocal chords, distinguished from the notes of stringed instruments?

It is not to be expected in an elementary manual that every subject, even of importance, should be noticed, but we should have thought that even in the briefest treatise on heat, some notice would have been taken of the "theory of exchanges," and yet we find no mention whatever made of it. Also a little more space than two pages might have been devoted to the electric telegraph, especially, first, as room has already been found for the description of the venerable three kinds of lever, and the antiquated three systems of pulleys (which are rarely seen except in text-books of physics); and, secondly, inasmuch as three times the space is taken up in the description of the air-pump and its modifications.

The good qualities of the book are so conspicuous, and its faults either so slight or so easily corrected by the teacher, that we have no hesitation in warmly recommending it as a good text-book for junior classes.

T. H. C.

OUR BOOK SHELF

Popular British Fungi; containing Descriptions and Histories of the Principal Fungi, both Edible and Poisonous, of our Country. Illustrated. By James Britten, F.L.S. (London: the *Bazaar* Office.)

THIS admirable little book forms an agreeable and popular introduction to a much neglected group of plants. Written in a pleasant easy style, it yet conveys a great deal of sound information. Mr. Britten having drawn on his imagination merely for the setting of his facts, not for the facts themselves. The different illustrations convey a tolerably accurate idea of the plants represented. The edible fungi are carefully described, and most useful hints and directions given as to the modes of cooking and preparing for table. The poisonous forms also receive a considerable share of attention, and the characters are carefully given, but even with all sorts of descriptions we cannot but think that there is always danger from such genera as Lactarius and Russula. Besides treating of the usual edible and poisonous fungi, Mr. Britten gives a chapter on Dry Rot, another on Luminous Fungi, and another on the Sphaeriacei. Throughout the whole book we constantly meet with quaint quotations from old authors. The book, then, is not only a very pleasant and readable one, but conveys a great deal of sound information on the subject therein treated.

Zeitschrift für das chemische Grossgewerbe. Herausgegeben von Jul. Post. II. Jahrgang. Heft I. (Berlin: Robt. Oppenheim.)

WE have already had occasion to express our high opinion of the value of Dr. Post's contributions to chemical technology. The present work is to the chemical manufacturer what the well-known "Jahresbericht" of Liebig and Kopp is to the scientific chemist. It attempts to give the technologist a systematic account of the latest advances in the several departments of manufacturing chemistry and the allied arts. As the various contributions are from the pens of men who, in the majority of cases, have made the matters upon which they write the objects of special attention and study, we can confidently recommend the work to the notice of our chemical manufacturers.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Indian Rainfall Statistics

SEVERAL correspondents in the *Times* and elsewhere have lately complained regarding the class of information furnished by the Government of India with reference to the famine. One of them pointed out that while the Indian official *Gazette* is filled with tables of food-prices, and other statistics, there seems to be no attempt to deduce the real significance of those figures, nor are any data of comparison offered by which the public might make deductions for themselves. I have just come across a very glaring instance of this. The Indian rainfall having been discussed a good deal this summer in your columns, I heard with some rejoicing that a long list of returns had been given in the Indian *Gazette* for stations in Madras and Bombay. The returns purport to bear upon the variation of rainfall in tropical India with reference to the cycle of sun-spots. As an old resident in Madras and Hyderabad, I think it would be difficult to produce a series of figures more irrelevant or more misleading with regard to the matter in hand. So far as I understand your articles, it is alleged that of the six famines in Southern India since 1810, five were caused by great droughts at the periods of minimum sun-spot frequency. It also seems to be alleged by you that the rainfall at Madras itself follows a cycle curiously coincident with the eleven-year cycle of sun-spots. These are two propositions distinct in themselves, and either of them is well worthy of investigation by the Meteorological Reporter to the Government of India.

The figures signed by that officer in the *Gazette* yield no information on the subject. He begins by completely mis-stating the case. Instead of testing the one statement as to whether drought and famine in Southern India have been coincident with periods of minimum sun-spots, or the other statement as to whether the rainfall at Madras itself forms a cycle coincident with the sun-spot cycle, he assumes that the question at issue is whether the rainfall at all the stations throughout Southern India shows a common periodicity coincident with the cycle of sun-spots.

Now, sir, it surely displays a gross ignorance with regard to the geographical facts of Southern India, to suppose that a periodicity, or any other feature of the rainfall of a place on the coast, like Madras, can be reproduced at the stations which the paper seems to take haphazard at the inner recesses beyond the Ghauts. I shall take, for example, only three stations in which I have resided, and whose monsoons I have seen. Madras derives about two-thirds, or twenty-nine inches, of its total rainfall from the north-eastern monsoon between October and December. The periodicity of its total annual rainfall is chiefly due to this monsoon. The Calcutta meteorological reporter compares this station with Secunderabad, where the north-

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eastern monsoon yields only three inches, and with Bangalore, where it yields only six or seven. I may add that he also compares it with Bellary, which derives only three inches from the north-east monsoon. It must be evident that any cyclic periodicity in the Madras rainfall which is chiefly derived from the north-eastern monsoon, cannot possibly be common to such stations as Bangalore, Bellary, and Secunderabad, which are scarcely reached by the north-east monsoon, and which derive an altogether insignificant rainfall from it.

I have only quoted three instances, but the paper is full of similar absurdities. The Government of India should not be surprised that we Madrassees are jealous of its interference and distrustful with regard to its ability to interfere for good, so long as it puts its official *imprimatur* on papers such as this. I do not suppose that Mr. Blanford, the meteorological reporter, is personally responsible for it. The explanation no doubt consists in the fact that the paper has been drawn up by some ignorant native clerk, and signed *pro forma* by his official superior. Any subaltern in a marching regiment who has crossed the Ghauts knows that the north-east monsoon cannot possibly exert a common influence upon the coast where it breaks in its first fury, and the walled-in plateau of Hyderabad or Bellary, where it scarcely reaches at all.

OLD MADRASSEE

Potential Energy

WHEREVER the fault may lie, your correspondent, "P. M.," has quite misapprehended John O'Toole, whose amanuensis I am, in every point on which he, "P. M.," touches, but one, viz., John's wish to place the potential energy in the force instead of in the body to be moved.

X.

Dublin, October 12

IN the capacity of poor P. I have suffered much at the hands of the doctors, and am glad to find in your correspondent "X." so competent an exponent of many of the difficulties with which the subject of energy, as generally taught, is beset. But while in the main I agree with the criticisms of "X." there are two or three points with reference to which I may perhaps venture an opinion.

It seems to be admitted (NATURE, vol. xvi. p. 459) by your correspondent that in the case of a moving body "the kinetic energy is undeniably in the body." This appears to me to lie at the root of the chief difficulty, for, as far as I can see, we have no more right to assert that a cannon shot possesses kinetic E. when it leaves the muzzle of the gun than to say that a clock weight possesses potential E. when it has been wound up; for it may happen that the shot is at rest relative (shall we say) to the centre of gravity of the physical universe. At any rate we are well assured that of itself it can do no work. Suppose the shot to find a home in the side of a ship; in entering the ship it does work, but the amount done depends upon the original motion of the ship, being greater if the ship were moving to meet the shot than if it were at rest; it also depends on the mass of the ship, for if the shot sensibly change the motion of the ship its own motion will be less altered by the impact. When a shot strikes a target, which we assume to be rigidly connected with the earth, the mass of the latter is so great that we may consider the target as fixed, and thus we have only to contemplate the mass and velocity of the shot. Simple problems of this nature were of course more inviting than those in which the mass and velocity of both the colliding bodies had to be taken into account; and thus it came to pass that the kinetic E. was attributed to the shot alone instead of being considered an attribute of the system consisting of the earth and shot together and due to the relative velocity of the two. Adopting this latter mode of expression, we may, if we please, suppose the shot to be at rest and the earth moving relative to it with the velocity of (say) 1,400 feet per second, and the energy of the system will be unaltered by our convention; but where should we be if in this case we supposed the kinetic E. of a rigid moving body to be an attribute of it alone?

Again, when we say that the kinetic E. of m units of mass moving with v units of velocity is $\frac{1}{2}mv^2$, this velocity must be measured with reference to some point or other which we for the time consider fixed. In order to obtain $\frac{1}{2}mv^2$ units of work from the body, we must bring it to rest relative to our point of reference, but in so doing we must take care that no motion is imparted to the point of reference itself, for if this be the case the body will come to rest relative to it without losing v units of velocity, and therefore without doing $\frac{1}{2}mv^2$ units of work. If our system consist only of two bodies, and work is to be done by bringing them finally to relative rest, this condition will be fulfilled only when the body which we choose to consider at rest is indefinitely great compared with the other, and only then may we represent the kinetic energy of the system by $\frac{1}{2}mv^2$, m being the mass of the smaller body and v its velocity relative to the other.

I think we shall avoid all difficulty if we define the kinetic energy of a system as *the energy which the system possesses in virtue of the relative motions of its parts*; we shall then never hear of the kinetic E. of a shot or other rigid body, except as an abbreviation for the kinetic E. of the system consisting of the earth and the shot, &c.

A precisely similar line of argument may be followed with reference to potential E. or the E. of position. The potential E. of a rigid body, whatever its position may be, is an absurdity. The very notion of position implies relation to other bodies, as we have no fixed points in space, and thus it is necessary, if only for this reason, that in speaking of potential E., at least two portions of matter which are capable of changing their position relative to one another should be taken into account. In the case of a raised weight, the system consisting of the earth and the weight possesses energy in virtue of the separation of its parts, and the system can be made to do an amount of work equivalent to this energy by keeping the earth fixed and letting the weight fall, or by keeping the weight fixed and letting the earth move up to it, or by letting each move to the other, as in nature. In any case the work done will be the same, though the time required may be very different; but according to the modes of expression complained of by your correspondent, I suppose that in the first case the potential E. belongs to the weight, in the second to the earth, and in the third it is divided between the two. Should we not avoid all difficulty by defining the potential energy of a system as *the energy which the system possesses in virtue of its configuration?* We should then never speak of the potential E. of a single rigid body, such as a raised weight, except as an abbreviation for the potential E. of the system consisting of the body and the earth. A strained elastic body, such as a bent bow, of course possesses potential E., for in this case the particles of the body have been moved relative to one another from their position of rest, and thus the configuration of the system has been changed.

Of course I agree with your correspondent that the potential energy of a system is just as truly *energy* as is its kinetic energy, and this brings me to the last point I proposed touching upon, viz., the term "energy of tension." Perhaps *energy of stress* might avoid some objections, since tension and its antithesis, pressure, have very special meanings; but surely in adopting such a phrase we can hardly say that the designation implies an "essential characteristic," while the term E. of configuration refers only to a "condition." In the case of two attracting bodies the potential E. is greatest when the attraction is least on account of the increased distance, and it depends not upon the actual attraction between the bodies in their existing configuration, but upon the attractions which are called into play in all the configurations assumed by the system as the bodies approach each other, and which therefore belong *in potentiality only*, to the system in its initial condition. If we define the potential of a point in the neighbourhood of a system of bodies as the amount by which the energy of the system would be increased by the introduction of the unit mass at this point, then so long as the mass is absent, the energy due to it can be only *potential*, but when the mass is placed there the energy is *actual*. In strictness, then, we ought not to apply the term potential to the energy thus introduced into the system. We, however, require some mark to distinguish this energy from kinetic energy, and the word potential serves to remind us of the condition of affairs before the mass was introduced. Again, we may have a stress as great as we please acting between the parts of a system without any consequent potential energy, so that the space "condition" seems to be at least as important as the stress "characteristic." On the whole I think the phrase *potential energy* preferable to energy of tension, stress, or configuration, although it is applied to energy which is as truly actual, and belonging to the material system, as is that of a shaft-impelled-against-an-ironclad.

Cambridge

W. G.

WITH reference to the question concerning the bricks, in NATURE, vol. xvi. p. 477, it is obvious that if a man lifts a brick down from a wall and places it on the ground, the *vis viva*

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